June 24, 1952

Dear Hayes:

Thank you for your interestingbnote of June 20. I am pleased to learn that lambda is not directly concerned with the UV effect.

I am not sure that the UV experiments can be unequivocally interpreted. It is not implausible that the UV may have two effects, so that some cells are phenotypically upgraded, others attenuated in their F+ character. Have you demonstrated a UV effect on TLB_1- F+? I am still undecided whether the mating reaction between two F+ strains represents a reaction between individual cells of differing, but F+ potency, or whether itvreflects a phenotypic variation so that some cells are effectively F-, as can be imposed on 58-161 by aeration. In view of the extreme infertility of some F+ x F+ combinations, I am inclined to the former view, but not decisively, and both processes may well be operating.

I do not share your deprecation of your previous theory. However you may alter your views, it did oblige me to think and argue more deeply than I otherwise would. Unfortunately, your scientific audience may not be able to keep up so readily with these developments, and I have still to "defend" recombination against the suggestion that lambda is the agent. (Of course, we are also somewhat culpable for having run into the confusing facts that a phage-mediated genetic transduction does indeed operate in Salmonella). At present, I am quite certain that our differences are purely verbal, and accede to the idea that different presentations may stimulate different kinds of experiments, and therefore be quite useful. If by one-way gene transfer, you imply a physiological or possible morphological anisogamy, I agree with you fully. But I am rather fully convinced from our older studies with diploids that the gene-deficiencies that arise during sexuality in K-12 arise after fertilization. The reasoning is quite complex, and therefore perhaps unreliable. When we get reprints of that monstrous Cold Spring Harbor Symposium, I'll send you a marked copy.

We have now about 50 new crossable E. coli strains. Many of these are F-, and will cross only with F+ K-12, unless F+ is transduced to them. Others carry a transducible F+ agent, and still others seem indifferent to the F+ system (possibly another argument against identifying F+ with the gamete). I am pondering a difficult decision: on which of these strains to gamble a good deal of work in hopes of finding a more straightforward sexual system than K-12, hopefully one in which the eliminations will not confuse the issue.

My wife has been looking further into the possibility of F+ filtrates. Like yourself, we have some unreliable portents of it.

I have also been looking into the cytology of the most compatible mating combinations. Like Klieneberger, I would conclude that the most striking result is that no very characteristic forms are to be found. I am inclined, without serious evidence, to the idea that mating consists of a conjugation rather like Paramecium, rather than copulation, and that the heterogamy consists of the migration of a nucleus from one cell to another. Such conjugations would be very difficult to detect and verify-- the main reason for my supporting the hypothesis.